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THE LOWELL OBSERVATORY, IN ARIZONA.

 BY EDWARD S. HOLDEN.

Mr. PERCIVAL LOWELL has lately established an observatory near Flagstaff, Arizona, at an elevation of 7300 feet above the sea.* According to press telegrams, this station is connected with the Harvard College Observatory (Director, Professor EDWARD PICKERING), and is to be under the charge of his brother, Professor WILLIAM PICKERING of the Harvard Observatory, who has lately been Director of the Harvard station in Arequipa, Peru.†

I have just received a copy of the Boston *Commonwealth* of May 26, 1894, which contains a long account of a paper read by Mr. LOWELL before the Boston Scientific Society on his new observatory and its plans for work. It is to be presumed that this abstract of Mr. LOWELL's paper is authoritative; but as it is not signed by his name, he may not hold the views there expressed. I have quoted below a few paragraphs from this account (the italics are my own), because they seem to me to be very misleading and unfortunate; and all the more so because they are very well written, and in some respects quite true. They are likely to be widely copied in other periodicals and widely read by many intelligent persons who are interested in the results of astronomical research, but who are not sufficiently instructed in the details of it to form independent judgments. It seems to be the first duty of those who are writing for such a public to be extremely cautious not to mislead; and especially to avoid over-statement. Conjectures should be carefully separated from acquired facts; and the merely possible should not be confused with the probable, still less with the absolutely certain. It

* See *Publications* A. S. P., Vol. VI, page 122.

† A letter from Mr. JOHN RITCHIE, Secretary of the Boston Scientific Society (received at Mount Hamilton, June 7) notifies the A. S. P. that the press-telegram printed in these *Publications*, Vol. VI, page 122, was incorrect. The telegram sent from Cambridge on February 12 stated that the new observatory in Arizona was to be a station of the Harvard College Observatory, whereas the fact is that the expedition is a private one, sent at the expense of Mr. LOWELL, who pays the salaries of the observers (they being on leaves of absence from Harvard) and who is the chief of the expedition. Mr. RITCHIE's letter concludes: "I enclose Mr. LOWELL's statement before the Boston Scientific Society, as published in the *Commonwealth* of Boston;" the enclosure is the article from which extracts are given above.

seems that the writer has not always observed these obvious rules. I have added to the quoted paragraphs a few remarks of my own, which are not intended as a criticism upon the writer of the article, but are primarily designed to point out statements which I think would be misleading to most readers. The quotations follow :

“With regard to the observatory’s plan of work, its main object is the study of our solar system. *This may be put popularly as an investigation into the condition of life in other worlds, including last, but not least, their habitability by beings like if (or ?) unlike man. This is not the chimerical search some may suppose. On the contrary, there is strong reason to believe that we are on the eve of pretty definite discovery in the matter.* In the first place, analogy warrants us in conceiving this little ball on which we dwell in the sea of space as no more the sole vehicle of intelligent life than it was once thought to be the pivot on which the whole cosmic system turned. Just as it is now known to be but one of many bodies revolving around the Sun, so, doubtless, is it but one of many worlds evolving in due course the phenomena of intelligent life. *If the nebular hypothesis be correct, and there is good reason at present for believing in its general truth, then to develop life more or less distantly resembling our own must be the destiny of every member of the solar family* which is not prevented by purely physical considerations, size and so forth, from doing so. In LAPLACE’S day, so much was but the natural deduction from celestial mechanics alone. Since then, however, we have got collateral evidence of another kind, from the astro-physical contributions of modern astronomy.

“The first bit of this *new* evidence may be said to have been the observation of the great red spot on *Jupiter*. Investigation into this led to the discovery that that giant globe still glows by its own inherent heat. Photometric observations of *Saturn* suggested a like state of things there. Finally, the spectrum of *Uranus* shows strange markings that point in the same direction. All of which goes to prove that in the case of the outer planets we are looking on globes midway in their course of development from stars to worlds. In the inner planets we see a very different state of things; a state more nearly that which our own Earth has reached. While in the Moon we gaze upon the last sad age of decrepitude, a world almost *sans* air, *sans* sea, *sans* life, *sans* everything.

“The next phase of the problem opened with SCHIAPARELLI’S great discovery, in 1877, of the so-called canals of *Mars*. Scouted at first by less penetrating observers, the existence of such canals has since been amply confirmed. Speculation has been singularly fruitful as to what these markings on our next to nearest neighbor in space may mean. Each astronomer holds a different pet theory on the subject, and pooh-poohs those of all the others. Nevertheless, *the most self-evident explanation from the markings themselves is probably the true one; namely, that in them we are looking upon the result of the work of some sort of intelligent beings*. In short, just as the great red spot on *Jupiter* implies that the planets may become habitable, the amazing blue network on *Mars* hints that one planet besides our own *is actually inhabited now*. What further heightens interest in the matter is that *Mars* has, from other considerations, undoubtedly reached a much later stage in planetary development than our Earth has yet attained, and that in gazing upon him we are in a sense peering into futurity.

“*Mars* is the most likely object for results to such planetary investigation. But there is evidence that something in the same general evolutionary line may be learnt from our other relatives in space—*Venus*, for example. From certain observations, it would appear that, belying her name, she is the most modest of all the orbs, keeping herself constantly cloaked in clouds—a state of things corresponding to our own carboniferous age. And we seem to be on the point of learning much more. *The increasing apertures of modern objectives*, taken in connection with what has recently been done and with the revelations of the spectroscope, *show that we stand upon the threshold of a knowledge of our closest of kin in the world of space, of the most important character.*”

The LOWELL Observatory is to be devoted to “an investigation into the conditions of life in other worlds, including their habitability by beings like, or unlike, man.” On this sentence I remark that any serious study of any planet by any means is, in its degree, a study of “the conditions of life” on that planet. A measurement of the angles of a crystal of fluorspar in a laboratory is part of a study of “the conditions of life” on the Earth. An accurate drawing of the planet *Mars* is, in its way, no doubt, a study of “the conditions of life” on that planet. But I submit that the phrase “the conditions of life”

is misleading. It holds out hopes that are very unlikely to be realized in the year of grace 1894, or during this century. What these hopes are is roughly indicated by the next phrase, "their habitability by beings like, or unlike, man. This is not the chimerical search some may suppose." The foregoing words seem to me to be especially misleading. Does the writer intend to say that there is any probability whatever that either of the two following questions can be settled within the next decade?

First.—Are *Mars*, *Venus*, etc., inhabited?

Second.—Are their inhabitants like (or unlike) human beings?

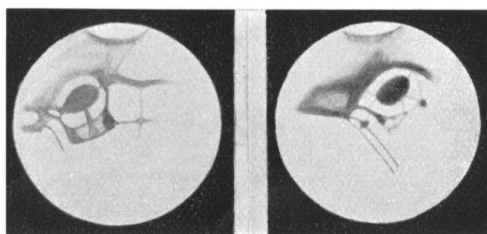
The words I have quoted seem to me to imply that there is a probability that these questions will soon be settled. I think many readers of the *Commonwealth* will so interpret them. But I also think that nearly every living astronomer will agree with me in saying, as I do, that there is no reasonable probability whatever of any such settlement at the present time. The problem is one for the considerably distant future, as has been well pointed out by Professor SEARLE in the *Publications A. S. P.*, Vol. II, page 165 (1890). What is there in the nebular hypothesis to show or even to hint that it "*must* be the destiny of every member of the solar family to develop life?" The nebular hypothesis has nothing whatever to say on the question of the *origin* of life; or on the evolution of organic beings of any kind. Its problem is—*given* a fiery mist containing all the chemical elements, and *given* plenty of time; *to account* for the present constitution and conditions of the inorganic constituents of the solar system. LAPLACE never dreamed of accounting for the beginning of organic life. DARWIN's book treats of the origin (*i. e.*, the differentiation) of *species*; not of the origin of *life*. SPENCER gives no authoritative account of the commencement of life. Sir WILLIAM THOMSON is obliged to bring organic life to the Earth on a meteorite in order to invent a beginning for organic evolution. Was not the meteorite very hot when it came to the Earth? It was certainly very cold in the stellar spaces. And where did this first organic cell originate, at any rate? Where did its "life" come from?

These questions, inexpressibly important as they are, have nothing to do with the nebular hypothesis, either in its old or new state. And the "new" evidence spoken of by the writer: how new is it? Most of it would be new to LAPLACE, no doubt.

But I think the average reader would derive the idea that he was hearing about very recent discoveries. Of course, the writer of the article knew how old some of these facts are; but I submit that he has, unintentionally no doubt, misled his audience. It *may* be that "the most self-evident explanation from the markings (on *Mars*) is probably the true one; namely, that in them we are looking upon the result of the work of some sort of intelligent beings." It just may possibly be true, though by no means "self-evident;" but I, for one, declare that I cannot find a scintilla of evidence in favor of that view. The writer is speaking of the so-called *canals* on *Mars*; these are long, dark narrow markings on a reddish background, and they are sometimes many hundreds, even thousands, of miles long, and seldom less than 50 miles wide. Sometimes they are visible, and sometimes they are not; they appear and disappear; sometimes they are double and sometimes single. Does the foregoing sound like a description of *canals*? If it were, should we think these waterways were built by intelligent beings like ourselves, or by madmen? Will not a canal 50 miles wide accommodate the shipping on *Mars*? And if more water surface is required, why not widen the canal? Why dig another one parallel to it? And, in the name of sane engineering, why fill it up every now and again only to open it the next season?

Again, how does the writer know that the dark markings on *Mars* are water and the red markings land? It may be so, but has it been proved? For my part, I only know a couple of arguments in favor of this hypothesis, and they are not conclusive. But the writer of the quoted paragraph assumes this to begin with, and I maintain that in doing so he is stating a probability as if it were a certainty; and he is stating it so well and easily as to mislead his readers. Finally, is it true that the large objectives of modern telescopes are especially well fitted to deal with faint contrasts like those on the planet *Venus*, so as to hold out a reasonable hope of new discoveries "of the most important character" regarding this planet? I doubt it, speaking for myself; and I can give at least one good reason for my conclusion.

With these few remarks, which could be carried much further, I leave the paragraphs in question. They seem to me to hold out hopes that are not likely to be quickly realized, and therefore to be suited to mislead rather than to lead the reader. They are all the more persuasive for being well written. But



August 14, 11^h 15^m.

August 17, 11^h 15^m.

DRAWINGS OF *MARS*, 1892.

By Professors CAMPBELL (left-hand) and HUSSEY (right-hand); Lick Observatory.

they tell only half-truths; and the particular half-truths that the whole human race would be delighted to have verified. It is the business of observatories—Mr. LOWELL's and others—to labor to obtain new light on such fundamental questions. It is the duty of all instructed observers to be scrupulously exact in announcing results for a popular audience. It seems to me that the writer I have quoted has not been sufficiently careful in this respect.

The very essence of the scientific habit of mind is conscientious caution; and this is especially necessary in referring to matters in which the whole intelligent world is interested—as the condition of the planet *Mars*, for example. I may take as an example the telegrams regarding *Mars* sent by cable from South America in 1892 by Professor WILLIAM PICKERING, who is to be the chief observer at the Lowell Observatory in Arizona. I quote two out of many such telegrams:

“NEW YORK, October 6, 1892.—The *Herald* correspondent at Valparaiso cables as follows: Professor PICKERING of the Harvard Branch Observatory at Arequipa says that he discovered forty small lakes in *Mars*.”

How does he know the dark markings are lakes? Why does he not simply call them dark spots? And is he sure there are forty?

“NEW YORK, September 2, 1892.—Professor PICKERING of Harvard College sends the following to the *Herald* from Arequipa, Peru:

“*Mars* has two mountain ranges near the south pole. Melted snow has collected between them before flowing northward. In the equatorial mountain range, to the north of the gray regions, snow fell on the two summits on August 5 and melted on August 7. I have seen eleven lakes near *Solis Lacus* varying in area from 80 by 100 miles to 40 by 40 miles. Branching dark lines connect them with two dark areas like seas, but not blue. There has been much trouble, since snow melted, in the Arean clouds. These clouds are not white, but yellowish and partly transparent. They now seem to be breaking up, but they hang densely on the south side of the mountain range. The northern green spot has been photographed. Many of SCHIAPARELLI's canals have been seen single.”*

*Several of these canals were seen not only single but double at Mount Hamilton. I do not know that they were so seen in Peru.

How is it known that there are two polar mountain ranges? How does he know that the flow will be northwards? And an equatorial range? Are not the gray regions so extensive that the description is, to say the least, indefinite? What is the evidence of "trouble" in the clouds? Is it certain that no clouds on *Mars* are white? How about the clouds "twenty miles high" reported by Professor PICKERING? Were they not white?

These and similar telegrams from South America regarding the happenings on *Mars* in the year 1892 were received by the astronomers at the LICK Observatory with a kind of amazement. We were observing *Mars* at that time (and continuously during the whole of each night) with a 36-inch telescope under good conditions. We have considerably more than a hundred drawings of the planet. Very many of Professor PICKERING'S conclusions could not be derived from our LICK Observatory drawings (which contained far more detail than his, however). Could they be justified by those he was making in Peru with a 13-inch telescope? This latter question could not possibly be settled in North America till we, also, saw the drawings on which Professor PICKERING'S conclusions were founded, and accordingly we waited. When the Peruvian drawings were finally published (*Astronomy and Astro-Physics*, 1892, page 668), it was at once obvious to all experts that they were not a sufficient proof of the reported discoveries. That our members may make comparisons for themselves, I insert some illustrations—first, a plate giving all of Professor PICKERING'S drawings which have been published; second, plates showing some Mount Hamilton drawings by Professors CAMPBELL and HUSSEY. We have over a hundred such, as I have said. Some of them have already been printed in the *Publications A. S. P.*, Vol. V, page 131, *et seq.*, and I refer to these also; as well as to others yet to appear. Good drawings are sometimes reproduced by engravers in a poor way. I have myself suffered from their neglect. But these drawings of Professor PICKERING'S are very well reproduced. It is obvious, at first glance, that the superstructure of announcement is too elaborate for the base of observations (so far as drawings like these represent the observations). All experts will agree that such diagrams as those made in Peru, unaccompanied by full verbal notes and measures, are not convincing evidence of important announcements. They are still (June, 1894) the only published

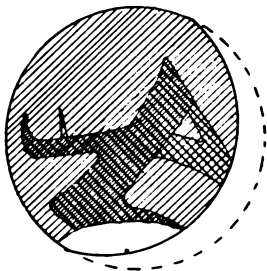


FIG. 1.
May 9, 21^h 05^m

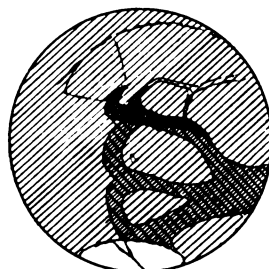


FIG. 2.
July 14, 16^h 50^m

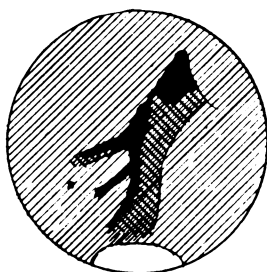


FIG. 3.
July 16, 17^h 45^m

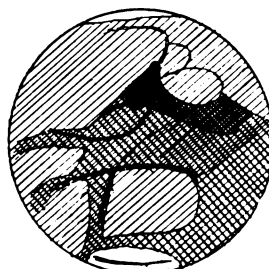


FIG. 4.
July 17, 15^h 50^m

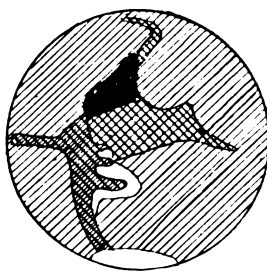


FIG. 5.
July 23, 17^h 30^m

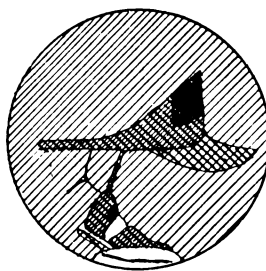
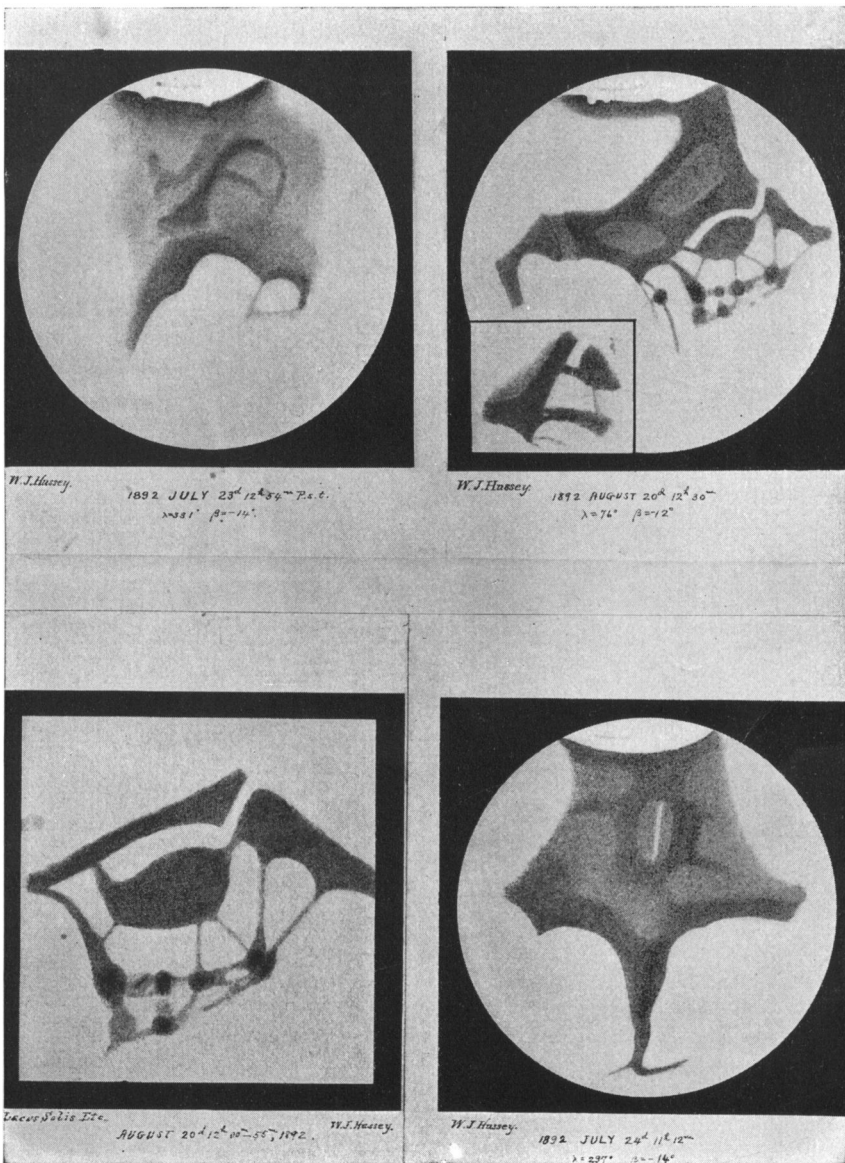


FIG. 6.
July 25, 20^h 40^m

MARS; MAY-JULY, 1892.

By Professor WILLIAM PICKERING at Arequipa, Peru.



DRAWINGS OF MARS, 1892.
By Professor HUSSEY; LICK Observatory.

evidence available regarding observations made two years ago, in July, 1892. Some of Professor PICKERING's results do not agree with the conclusions so far reached at Mount Hamilton. For that reason, we are justified in doubting them until convincing evidence is at hand, as it certainly is not now.

Professor PICKERING's observations of clouds "twenty miles high" on *Mars* are discussed by Professor CAMPBELL in *Publications A. S. P.*, Vol. VI, page 108 (1894).

I may quote another telegram from Peru :

"NEW YORK, October 14.—The *Herald* received the following cable from Arequipa, Peru, to-day: '*Jupiter's* fourth satellite is very dark now. The third satellite is girdled by a very dark belt in the northern hemisphere, inclined 20 degrees to the orbit. It can be best seen during the transit. The second satellite is probably not spherical. The first satellite is egg-shaped, and revolves end over end, and nearly in the orbital plane. Its period is twelve hours and fifty-five minutes. PICKERING.'"

In other places Professor PICKERING has given details regarding his observations of the behavior of the satellites of *Jupiter*.

Professor PICKERING's conclusions as to the first satellite of *Jupiter* may (possibly) be true; but continued observations at the LICK Observatory, for some years past, do not lead us to accept them. Professor PICKERING's observations on *Jupiter* were made in 1892. They have not been verified by anyone during the very favorable opposition of 1893. I can see no reason why any one of a dozen large telescopes, now active in the northern hemisphere, should fail to see in 1893 what was seen with a 13-inch in Peru in 1892. The observations of Professors SCHAEBERLE, BARNARD, CAMPBELL, and my own, at the LICK Observatory distinctly contradict those made in Peru. In No. 3229 of the *Astronomische Nachrichten* (which was received after this paper was written) is a note from Professor PICKERING regarding the bright belt on *Jupiter's* first satellite which Professor BARNARD described in the *Monthly Notices*, R. A. S., for January, 1894, and which was observed by him during 1893. Professor PICKERING says that this bright belt is not "a permanent one, for it certainly did not exist at the time of the opposition of 1892." By this he can only mean that he did not see it in Peru with the 13-inch equatorial, and that so far as his observations go, it did not exist. If he had turned to the *Publications* of the A. S. P., for 1891, pages 355-357, he would have seen that Professors

SCHAEBERLE and CAMPBELL regularly observed the belt during September and October, 1891. Its existence was fully demonstrated at this time, which is the reason why there are no further published observations by these gentlemen. Do not the published observations of this belt at the LICK Observatory during 1891 and 1893 indicate that the Arequipa observations of 1892, which fail to show the belt, are, in so far, erroneous and incomplete? If this bright belt, which is obvious in our 36-inch telescope, was not visible in the 13-inch telescope at Arequipa, is it not likely that most of the differences between the Mount Hamilton and Arequipa observations are to be laid to insufficient optical power at the latter station? Such a solution is the most natural one to adopt, at least until adequate proof is brought forward (as it has not yet been) that this supposition is incorrect.

In this connection I refer to a paper by Professor SCHAEBERLE, printed in this number of the *Publications A. S. P.*, and to his remarks on the shape of the shadows of the satellites of *Jupiter*. They are seen, at certain times, to be twice as long as they are broad, and this phenomenon is regularly observed here. I have myself photographed this appearance. I believe it was not noticed in Peru, which is strange, considering how much more difficult observations are reported.

At the coming opposition of *Mars* (1894) Professor PICKERING will have the use of an 18-inch telescope. An 18-inch is considerably more powerful than a 13-inch, but it is considerably less so than our 36-inch. It is to be hoped that the observations to be made in 1894, at the Flagstaff and Mount Hamilton Observatories, will agree better than those made in 1892, at Arequipa and Mount Hamilton. If they do not, it cannot be considered unreasonable on our part to maintain that our work at the LICK Observatory is deserving of the greater weight (since we have the better opportunity), at least until convincing proof is brought forward to the contrary. Such proof we shall be the first to accept. Evidence of the kind which is now available in support of the Arequipa observations of 1892 will not be sufficient.

Finally, I wish to disclaim, in the frankest manner, any desire to uselessly criticise the paper in the Boston *Commonwealth*, or the observations of Professor PICKERING in Peru. I have felt it imperative to remark on what seems to me an unfortunate and misleading tendency in the article referred to; and also to point out that the Peruvian observations of *Mars* and *Jupiter* (which

might seem to support the conclusions of the article) are themselves in serious need of confirmation. I trust I have been able to do this in all courtesy; and at the same time in a manner which will carry conviction.

EDWARD S. HOLDEN.

THE LICK OBSERVATORY, June 1, 1894.